came under my notice at Brown's Town, viz. two perfectly black parents having a family all pure albinos.

Kingston, Jamaica, May 26 THOMAS CAPPER

### Singular Behaviour of a Squirrel

A NEIGHBOUR of mine, whose cottage is thickly surrounded with trees, observed a squirrel, during the severe weather of winter, occasionally stealing food from the troughs set out for the poultry. At first it caused great commotion among the birds, but latterly they were less uneasy in its presence. Taking an interest in the wild creature he began to lay out refuse food for it, including bits of ham, which it greedily appropriated. Getting more courageous, it ventured within doors. After a time it got caught in a trap set for rats underneath the bed. Being freed from its irksome position it was thought that the squirrel would venture no more within doors. Neither the incident of the trap nor confinement for some time within a cage availed to restore to it its original shyness. With the coming of summer its visits have been less regular, but occasionally it looks in still. May not a habit like this, affecting only one out of many, be looked upon as corresponding to a "sport" in the vegetable world, and shed some light on the subject of the domestication of animals? The squirrel seems to have been quite a wild one to start with, for there is no one in the district who had been in the habit of keeping one as a pet. J. SHAW

Dumfriesshire

### Hot Ice

IN reply to a very interesting letter on this subject recently published in NATURE (vol. xxiii. p. 504) by Dr. Oliver J. Lodge, I wish to express my views of the theoretical and practical possibility of the experiment of Dr. Carnelley. I wish to start from some well-known principles accepted by everybody acquainted with the mechanical theory of heat and its applications. According to these principles the volume "$v$" (and also the total amount of internal energy) of water can be expressed as a function of its pressure "$p$" and temperature "$t$"; $v = f(p, t)$. The form of this function, which we need not discuss here, will change with the state of aggregation, so that we shall have three different equations expressing the volumes of water in the solid, liquid, and gaseous form.

$$\left.\begin{array}{ll} v = f_{\text{i}}(p, t) \ \dots\ \dots & \text{ice} \\ v = f_{\text{ii}}(p, t) \ \dots\ \dots & \text{water} \\ v = f_{\text{iii}}(p, t) \ \dots\ \dots & \text{vapour} \end{array}\right\} \ p \text{ and } t \text{ being considered independent variables.}$$

Geometrically the volumes of ice, water, and vapour will belong to three different surfaces extending between certain limits. Thus the surface $v = f_{\text{i}}(p, t)$, which represents the volumes of ordinary ice, is situated between the limits $q\,p$, $l\,m$, $m\,d$; the surface representing liquid water lies between $m\,n$ and $m\,d$, though it may be extended a little on either side of these limits, if it applies to water heated or cooled over its regular boiling or freezing temperatures, which are situated along the lines $m\,d$ and $m\,n$.[1] The values of $p$ and $t$, which belong to $m\,d$ and $m\,n$, will satisfy two equations—$\phi(p, t) = 0$ and $\psi(p, t) = 0$. At these points the water will change its form of aggregation and pass over in the state of saturated vapour along the line $m\,n$ [equation $\psi(p, t) = 0$], or into ice along $m\,d$ [equation $\phi(p, t) = 0$] in a continuous and *reversible* way. At any other point, which is not situated on $m\,n$ or $m\,d$, water may also be liable to change of aggregation, but this process will not be *reversible*. The line $m\,n$, where the surface $v = f_{\text{ii}}(p, t)$ breaks up and liquid water changes into vapour, is the *curve of tension of saturated vapour* contained in the renowned table of Regnault. The boiling-points of water under varying pressure are situated on $m\,n$, and may be found by solving the equation $\psi(p, t) = 0$. At the point $m$ ($p = 4{\cdot}6$ mm., $t = -0^\circ{\cdot}0078$ C.) the line $m\,n$ terminates, but is continued by $l\,m$ [equation $\chi(p, t) = 0$], along which the vaporisation of ice takes place in a reversible way. According to the table of Regnault there is no sudden rupture at the point $m$, the pressure of saturated vapour at 0° C. being identically the same, if the vapour is in contact with water or with ice. The differential coefficients $\dfrac{dp}{dt}$ of the functions $\phi(p, t)$, $\psi(p, t)$, and $\chi(p, t)$, or the tangents to the lines $m\,d$, $m\,n$, and $m\,l$ are found by application of Carnot's Theorem to be of the general form $\dfrac{dp}{dt} = \dfrac{A\,r}{[s - s_1][273 + t]}$ [$r$ = latent heat; $s$ and $s_1$ = the specific volumes of water in two different forms of aggregation].

The point $m$, where $m\,n$, $m\,d$, and $m\,l$ unite, is of particular interest. J. Thomson called it "the triple point," and Guldberg the "Fällespunkt" of water. Lately (in *Berichte*, 1880) I ventured to call it the "absolute point of sublimation," not because I wished to introduce a new term for a well-known scientific object, but only to point out some important consequences of the phenomenon just then announced by Carnelley, of which Prof. Lothar Meyer of Tübingen had published an interpretation different from mine. This point $m$, situated $-0^\circ{\cdot}0078$ C. below the ordinary freezing-point of water, is really the upper limit of sublimation, because at any higher temperature ice first changes into water before it evaporates. At $-0^\circ{\cdot}0078$ C., where the boiling- and melting-point of water coincide, a real sublimation of ice begins, provided that the barometric pressure does not exceed 4·6 mm. (= "the critical pressure" of Carnelley).

Now according to the discovery of Dr. Carnelley, ice at pressures lower than 4·6 mm. would exist by temperatures up to $+178^\circ$ C. Thus the surface $v = f_{\text{i}}(p, t)$, which we have hitherto supposed to be inclosed between the limits $q\,p$, $q\,l$, $l\,m$, $m\,d$ would extend far beyond $l\,m$ nearly up to $k$, but always at pressures smaller than 4·6 mm. Geometrically this new and unforeseen

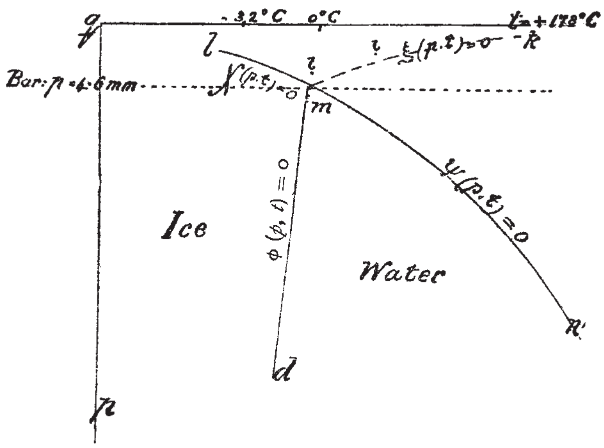


FIG. 1.

extension of the surface of ice is represented by the area $l\,m\,k$. Here the process of Carnelley, whereby ice of low pressure is heated to astoundingly high temperatures, would go on. The area $l\,m\,k$ would of course be entirely a *terra incognita* to the science of the present day, but there is nevertheless no theoretical objection why the surface of ice $v = f_{\text{i}}(p, t)$ should not extend farther than to the limiting line $l\,m$ pointed out by Regnault. Confiding in the experimental proofs already furnished by Dr. Carnelley, I concluded (*Berichte*, 1880): if the surface of ice really extends upwards to about $+178^\circ$ C. there must be a limiting line $m\,k$ to the area $l\,m\,k$, since this area cannot extend so far as to the dotted line in the figure indicating the critical pressure = 4·6 mm. At this new limit, $m\,k$, corresponding to an equation $\xi(p, t) = 0$, the vaporisation of the "hot ice" may go on in a reversible way, just as liquid water gives up saturated vapour at those pressures and temperatures which belong to the line $m\,n$ (equation $\psi(t, p) = 0$). The line $m\,k$ would in many respects be the continuation of $m\,d$ (just as $m\,l$ forms the continuation of $m\,n$), but naturally the symbols entering the equation of its differential coefficient $\dfrac{dp}{dt} = \dfrac{A\,r}{(s - s_1)(273 + t)}$ must change their signification on the other side of the point $m$, so that $r$ here would represent the latent heat of *vaporisation* of the hot ice, $s$ its specific volume, &c. I did not expressly mention this in my paper in the *Berichte*, because I thought it unnecessary. This omission on my side may probably have misled Dr. O. Lodge as to the real meaning of my words, since he declares my opinion that an equation $\xi(p, t) = 0$ having a differential

[1] The surface corresponding to the volumes of aqueous vapour $v = f_{\text{iii}}(p, t)$ is not sketched in the figure, which gives only *the projection* of the surfaces on the plane of co-ordinates $p$ and $t$, not the real situation of these surfaces in space. The reader will also observe that the limiting lines $m\,n$, $n\,d$, $l\,m$, $m\,k$ are the *intersections* of vertical cylindrical surfaces ("Uebergangsflächen") with the plane $p$, $t$.

coefficient $\frac{dp}{dt}$ of the general form above mentioned will still exist for pressures below 4·6 mm. and temperatures higher than −0·0078° C., where it is now supposed to have a *point d'arrêt*, to be "naturally erroneous," an assertion which I hope the learned English inquirer may feel inclined to withdraw after the preceding explanation. Of course the necessity of my conjecture may be disproved by facts—if there really should exist no "hot ice"—but nevertheless it deserves to be discussed as well as the opinion of Dr. Lodge, who considers the existence of hot ice to depend entirely on an irreversible process of vaporisation from the ice resembling the evaporation of water in an atmosphere which is not saturated with damp. This observation only regards the experiment, not the theory. I fully admit that Dr. Carnelley's experiment is carried on in an irreversible way, but that is the case with every distillation or sublimation which is *practically* performed. Nevertheless there exists a line $m\,n$ where the liquid water changes gradually and reversibly into saturated vapour, and that may be the case also with the hot ice at the limit $m\,k$. Any irregularity in the operation will not exclude the possibility of the existence of an equilibrium established by nature. The difference of temperature between the hot ice in the experiment of Carnelley and the cooled vacuum bottle is no objection to this, because we *might* carry on the operation in quite another way, dispense with the vacuum bottle and the cooling mixture, and keep up the necessary minimum of pressure, which is the only *sine quâ non*, by means of a powerful air-pump. In a similar experiment (with $HgCl_2$) Dr. Carnelley operates in that way. Dr. Lodge, on the contrary, is convinced that ice, which has once passed the triple point $m$ can sustain whatever augmentation of pressure and temperature may be applied to it. We may destroy the vacuum, so scrupulously kept up by all experimenters, and allow the air to enter through hot pipes, nevertheless the ice will not melt. I interpret this in the following way:—Dr. Lodge admits that the surface $v = f_i(p, t)$ extends over the limit $l\,m$, and even surpasses the critical pressure 4·6 mm., the vaporisation of the hot ice going on irreversibly the whole time.[1] This is indeed an interesting hypothesis, which well deserves to be tested by experiments, but yet lacks any foundation from facts. I therefore think that the proper method of resolving the entire problem would be:—

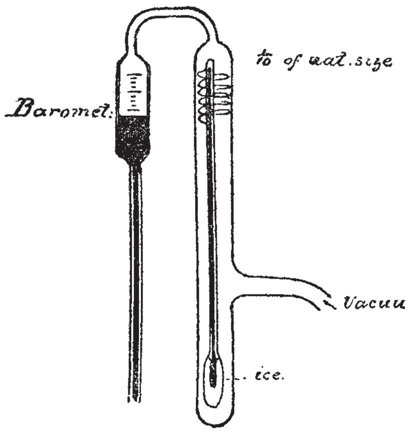


Fig. 2.

1. To try (by experiment) if ordinary ice under low pressure by sufficient supply of heat can be made to pass over the limit $l\,m$ and assume higher temperatures than those corresponding to the equation $\chi\,(p, t) = 0$ (or Regnault's table).

2. If this should be the case it remains to ascertain if the vaporisation of the "hot ice" tends towards any limit ($m\,k$), where this process becomes reversible, saturated vapour being formed [my conjecture], or if there is no such limit [theory of Dr. Lodge].

The apparatus employed (see Fig. 2) differs from those recently used by Messrs. Boutlerow, McLeod, L. Meyer, &c., only by its combination with a barometer, by means of which the variation of the pressure of the vapour given up by the ice during the whole process could be exactly measured. The only drawback to this was that the barometer of the apparatus did not *instantly* indicate the variation of the pressure, because the *upper* part of the barometer was made of a very wide glass tube to avoid the influence of capillarity. The effect of the vacuum, which consisted of a 4-litre glass bottle, was very powerful, since the full heat of two strong gas-lamps, each furnished with three pipes, must be employed on the outside of the glass tube in order to raise the temperature of the ice covering the bulb of the thermometer from −15° C. or −11° C., up to 0° C. The result of the experiment was (the ice being heated *only by radiation* from the glass tube):—By intense heating the temperature of the ice slowly (in about six minutes) rose from −11° C. to 0° C., when it became *constant* for half a minute. Then the ice melted, and the first drop of water falling upon the bottom of the heated glass tube was sufficient to crush the apparatus. During the process of heating the niveau of the mercury in the barometer-tube constantly fell, the internal pressure augmenting as the temperature of the ice rose. It was quite impossible to raise the temperature of the ice without simultaneously augmenting the pressure.

| | *Experiment I.* | *Experiment II.* |
|---|---|---|
| The ordinary barometer showed | = 756·8 mm. | = 771·1 mm. |
| The barometer of the apparatus showed | = 755·0 mm. | = 769·5 mm. |
| The initial pressure in the apparatus | = 1·8 mm. | = 1·6 mm. |
| The initial temperature of the ice | = −11°·0 C. | = −15° C. |

| Experiment I. | | Experiment II. |
|---|---|---|
| By heating the temp. rose to = −8°; | the press. = 2·5 | $t$ = −9° p. = 1·8 mm. |
| ,, ,, ,, = −6°; | ,, = 2·90 | $t$ = −6° p. = 2·6 mm. |
| ,, ,, ,, = −4°; | ,, = 3·5 | The mercury in the stem of the thermometer separated by the heat. |
| ,, ,, ,, = −0°; | ,, = 4·0 | |
| The ice melted. | | |

TABLE OF REGNAULT.

Tension of saturated vapour at—

$t = -20°$ C.; $p = 0·927$
$t = -15°$ C.; $p = 1·700$
$t = -10°$ C.; $p = 2·093$
$t = -5°$ C.; $p = 3·113$
$t = -0°$ C.; $p = 4·600$

I also repeated the experiment of Mr. Hannay by substituting a little sealed tube containing frozen water under atmospheric pressure, instead of the bulb of the thermometer. I found, in accordance with Mr. Hannay, that the enveloping ice melted before the ice in the tube.

After the experiments published by Messrs. McLeod, Boutlerow, L. Meyer, v. Hasselt, de la Rivière, and Hannay, I think it may be considered as a matter of fact that ordinary ice under low pressure cannot be heated over 0° C. In the experiments I. and II. I vainly tried to raise the temperature of the ice without simultaneously augmenting the tension of the vapour in the apparatus. It seems probable therefore that the area corresponding to $v = f_i\,(p, t)$ does not extend farther than to the limit $l\,m$ [equation $\chi\,(p, t) = 0$], since the temperature of the ice and the tension of its vapour vary almost exactly in the ratio given by Regnault's table, which in Fig. 1 is represented by the line $l\,m$. We may conduct the heating of the ice so as to follow almost continuously the line $m\,l$ [Experiment I.] without ever being able to pass over it or to reach temperatures situated beyond $l\,m$ (*i.e.* in the area $l\,m\,k$). Still I think these experiments to be strictly convincing only in the case of *ordinary ice*. Nobody has yet repeated Dr. Carnelley's experiment exactly in the same way as Dr. Carnelley himself. In his experiment the ice on the bulb of the thermometer is formed not by the freezing of a quantity of water, but by the sublimation and condensation of icy vapour to thin layers. It *may* be possible that ice, by sublimation under low pressure, changes into another allotropic modification, just as the red modification of $HgI_2$ is changed into yellow iodide by sublimation. In this case we may foresee the existence of a new surface, $v = f_{iv}\,(p, t)$ on the other side of $l\,m$. For, according to the principles of the theory of mechanical heat, there ought to be a new function, $v = f_{iv}(p, t)$ for every new allotropic modification of a body which, geometrically, is represented by a surface (?? in the figure). We are scarcely authorised to deny the possibility of the existence of hot ice, since Dr. Carnelley has obtained several pieces of ice, which

[1] *Or:* Dr. Lodge supposes that the volume of ice which has once passed the limits, beyond which liquid water cannot exist, is totally *independent* of the temperature and pressure. In this case no theory can be applied to account for the existence of hot ice, because every theory must start from the assumption that there exists a certain relation between the variables $v$, $p$, and $t$, and that the volume of ice, as long as it *is* ice, is not *arbitrary*, but regulated by an equation $v = f_i(p, t)$. Therefore I do not think that this explanation can be in accord with the views of Dr. Lodge.

did *raise* the temperature of the calorimeter. I have tried to repeat this experiment, but I never could obtain the whole bulb of the thermometer entirely covered with sheets of sublimated ice, and without this the experiment will be illusory. Many times I obtained lozenges of sublimated ice, which did adhere very strongly to the bulb of the thermometer, until it showed + 35° or + 40° C. Then the lozenges generally fell off. I do not consider this to be any deciding proof, as it may depend on a phenomenon similar to that of Leidenfrost, nor do I think it very probable that ice really can exist at those temperatures, but if that should be the case the simplest manner to account theoretically for the existence of hot ice would be to assume a new allotropic modification, since it may be regarded as a matter of fact that *ordinary ice* cannot be heated over the limits pointed out by Regnault. If this should be the case I think that the importance of the discovery of Dr. Carnelley could hardly be overrated.

Upsala, May 28 OTTO PETTERSSON

TEMPERATURE OF RAIN-WATER.—"A Subscriber" asks where he can find records of the temperatures of rain-water when falling, and of the earth a few inches below the surface, during any or all the months of the year.—As regards the British Islands, the most extensive and long-continued observations on the temperature of the soil are those published by the Scottish Meteorological Society since 1857. A *résumé* of the first five years' observations was published in the Society's *Quarterly Report* for October–December, 1862. In the Society's *Journal* (vol. i. p. 320) is a discussion of valuable series of observations made on the temperature of drained and undrained land at various depths; also in *Journal* (vol. ii. p. 273, and vol. iii. p. 211) discussions of hourly observations on the temperature of the soil and of the air at different stations in Scotland. With respect to the temperature of falling rain, little, if anything at all, quite satisfactory, has been accomplished, the practical difficulties in the way being either not apprehended by the observer or not satisfactorily disposed of. Our correspondent may also consult with advantage the publications of the various Continental organisations for the prosecution of forest meteorology.—ED.

## NOTES ON ALGÆ[1]

THE publication of the second part of Bornet and Thuret's volume on Algæ seems a fitting opportunity to notice it in some little detail. While the First Part, published in 1876, treated chiefly of the red Algæ, by far the larger portion of this Part treats of the Nostocs; while the First Part contained a good deal of the notes of Thuret, the present is practically the work of Bornet, and the drawings are in almost every instance from this author's pencil.

Under the modest title of Notes, we find in this handsome quarto volume, of over a hundred pages and twenty-four plates, in addition to notes on the higher Algæ, a most exhaustive treatise of a very interesting group of simple Phycochromaceous plants.

The illustrious Thuret had laid the foundation of a knowledge of the Nostocs; his friend Bornet has built thereon a very solid structure. He has not attempted to write a complete monograph of the group, including therein all the "book" species, but having had access to most of the published collections of dried Algæ, to the collections of the Paris and Dublin Herbaria, and to the original types of de Brebisson, Lenormand, Montague, Harvey, Grunow, and Le Jolis, he has performed wonders in the way of clearing up a most tangled synonymy.

It might shock the nerves of some botanists to recommend that all defective descriptions of Algæ—of which no original type specimens exist—ought to be overlooked. We believe, however, that it would be for the advantage of science that such a step should be taken. We may here mention that the collection of Dr. Hassall, from which most of the drawings of that author's "History of British Freshwater Algæ" were made, has been long since dispersed; so far as the Nostocs are concerned, this does not much matter, as all the species were described from authentic specimens still attainable.

[1] Notes Algologiques: Recueil d'Observations sur les Algues, par M Ed. Bornet et G. Thuret. Deuxième fascicule. Paris, 1880. G. Masson.

The Nostocs (this name is traceable to Paracelsus—"Sic etiam quicquid aër gignit et ex aëre est vivitque vel oritur ut Tereniapin, Nostoch," &c.—and yet no one seems to know its meaning) are well-known plants. One common species makes its appearance on lawn or garden walks in summer or autumn in the form of olive green (rarely bluish), irregular, and more or less shining masses. It is strange to hear the guesses that are made as to the nature of these. We have had them sent to us as the "peculiar spawn of earthworms," and again as the eggs of some foolish frog that had mistaken dry land for water. Some species delight in moist banks over which water continually trickles; some live a wholly aquatic life on stones in streams. The species have an enormously extended, but not yet accurately defined geographical area. As to size, they vary much, some being scarcely visible to the unassisted vision, some forming masses as large as the upper joint of one's thumb.

The details of such a volume as the one before us are too special to be of general interest, so we rest satisfied with indicating the chief contents. The genus Nostoc is treated of very fully; the reproduction of the species by hormogones and by spores is well illustrated. Instead of the term trichome, we would have preferred that of filament, for the former has now obtained such a common usage in another sense among botanists. Despite a wonderful uniformity in their structure, the spores seem to furnish good diagnostic differences. It is unfortunate that they are not as yet known in all the species, while in some they are difficult to hit off. Twenty-nine species are formulated. Carefully-conducted culture experiments, carried on over four years, have proved that Nostoc cells found within the cells of aquatic plants (Potamogeton, &c.) will develop into regular Nostoc colonies, which latter have been traced to the spore-producing stage.

Four species of the genus Nodularia are described and figured. This genus is better known under its more familiar title of Spermosira. *Nodularia litorea* is a somewhat remarkable species. In July, 1874, M. Crié was commissioned to make an inquiry into the cause of a noisome smell proceeding from the little lake of Deauville (Calvados). It would appear that for some years this district had been a regular focus of maladies, and those living near it had remarked that the fœtid odour perceived at times came from a reddish matter which periodically covered the surface of the water. M. Crié soon found that this consisted of ruddy masses of this Nodularian, spreading over the surface of Ruppia, and that its periodic decomposition—at the moment of greatest heat and lowest water—was the cause of the stench. The perfect remedy was found in guiding a stream through the little lake or pond, and thus preventing the too rapid growth of the Alga

Of the other genera treated of we must mention Lyngbya, Plectonema (for *Conferva mirabilis* of Dillwyn), Scytonema (twenty-one species, of which a provisional analysis is given. Some twenty-one species (?) are included under *Scytonema thermale* (Kutz.), and a very important Appendix gives a list of plants determined NOT to belong to the genus, though referred to it); Calothrix (several of our native species are figured and described); and lastly, Glœotrichia (of which six species are admitted).

Enough has been written to prove how valuable an addition to our works on the lower algal forms this volume is. To the worker on this group—ever increasing in interest—this contribution to our knowledge of it will be very welcome. Such will call to mind, too, that there are still lower and more confusing forms of these Algæ, and will be glad to hear that it is probable that the same patient and clever hand hopes shortly to have reduced even them to something like order. E. P. W.